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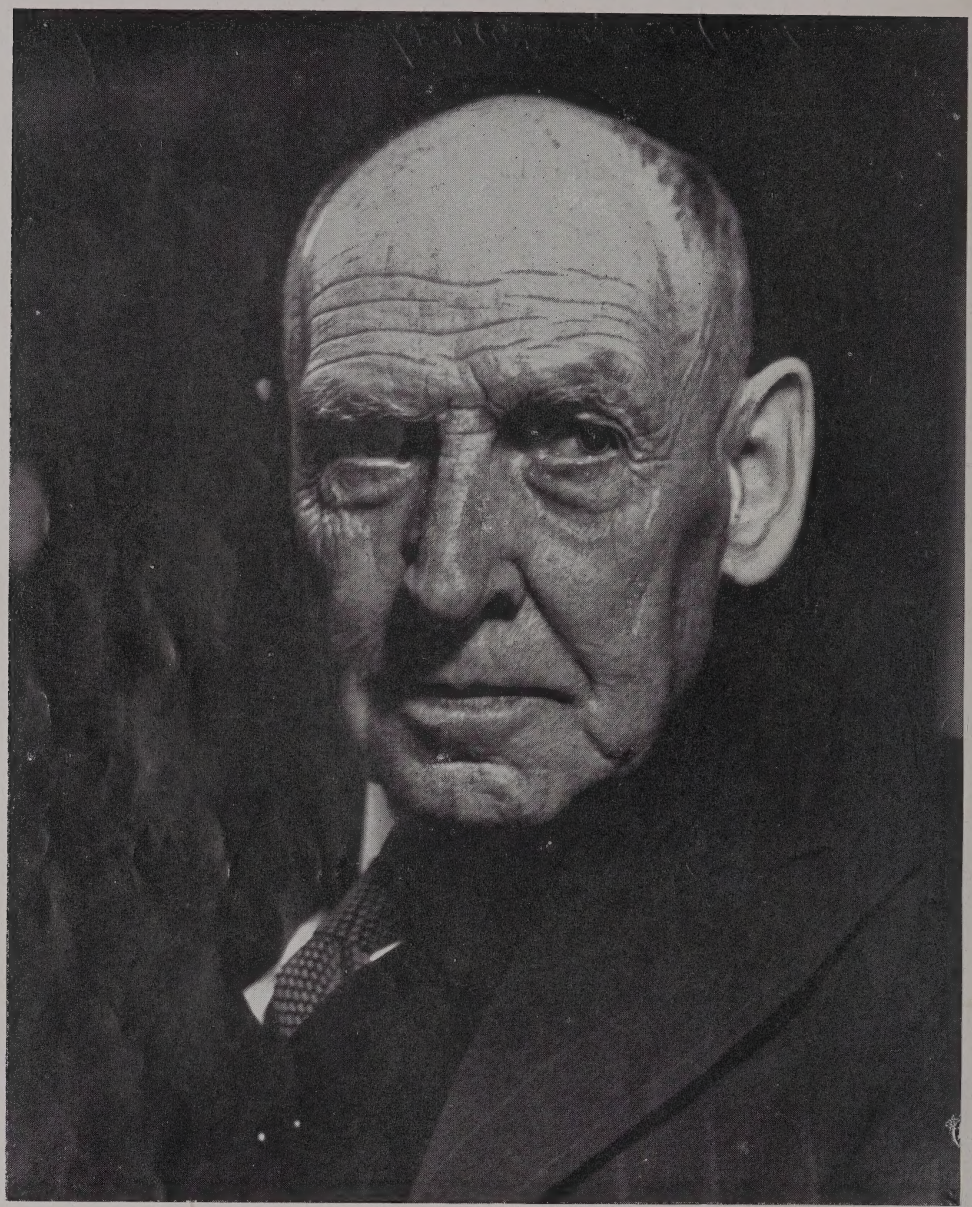
AN AUTOBIOGRAPHY



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An Autobiography

By MILLAIS CULPIN

My approach to psychology has been devious enough to call for the explanations demanded by your editor, so here they are. Born in 1874, second of six siblings, I lived as a boy at Stoke Newington, in a Georgian house facing a main road along which waggons loaded with farm produce rumbled on their way to town in the mornings,to return later bearing the dung from the stables of London. My father was a general practitioner and his family belonged to Hertfordshire; my forename belongs to the family and has no artistic connexion. Seven years I spent at a local school of a kind now extinct, the head of which had a simple educational plan – to give us stuff to learn from a book and up to six handers if we didn't know it, licking his thumb between the strokes. I fared better than the dull ones. I had a good memory, I read all I could get, including my father's *Lancet*, and I still regard Euclid's literary style as ideal. Those years made me a doubter of authority, slow to recognize when it was on my side and even then diffident and mistrustful, though at fifteen a move to the Grocers' Company's School did something to alter this. There I found myself comfortably in the lower sixth, but my Latin had not responded favourably to the methods described above. An exercise from *Bradley* sent me to the Head's room with a half-serious warning from my class-mates about a tanning, which I was ready to accept as the fortune of war. But the Head, the Rev C. G. Gull, started off with friendly praise for my success in making more mistakes than there were words in the exercise, explained a few, and wound up with the warning that every word in *Bradley* was a pitfall. I walked on air as I left that room and when *Bradley* again fell due I sat up half the night wrestling with it. At the end of the next term I won the form scholarship, and I have never heeded discussions about the disciplinary value of learning the classics, for my mind was made up in those days. That I should have been plagued by the linguistic freaks of ancient Romans, that I am still able to recite slabs of the 'gender card' with its arbitrary rules and more arbitrary exceptions, are, to my adult mind, all of a piece with the unquestioning receptivity of the herd that permits world wars. *Arnold's Latin Prose Composition revised by Bradley* should be in the museum of the R.S.P.C.C. Horace was different. There was music in him, and human feeling, and love of nature. *Solvitur acris hiems grata vice veris et Favoni* still helps me through the dark days and always comes true.

By this time I had found my way into a group of lads keen on ento-

mology and I used the scholarship money to join in a trip, the first of many, to Wicken Fen, the bug-hunter's Mecca. Three years of field work and reading in this subject, directed entirely by ourselves, gave a contact with biological principles and scientific terminology the value of which I appreciated only in later years. Meanwhile school went well and I matriculated in January, 1891, my only school companion being R. W. Chambers. Years later we found ourselves occupying academic chairs in the same street, he having become Quain Professor of English at University College.

Now came a great change. My father was over-worked, and London fogs, the yellow sort that made me weep, made him more chesty each winter, so he sold his practice and took us all to Queensland. He set up practice in Brisbane and I tried various ways to earn a living, spending three months at a gold rush where an illiterate but skilled miner from Gympie took charge of my fortunes, taught me how to use a shovel and cook a damper, and was always ready to back his practical experience against my matriculation mechanics. We raised several tons of stone that showed gold when 'dollied' but it would not have paid for cartage and crushing. Then I got up North and took on the job of schoolmaster at the Laura, a township on Cape York Peninsula about 15 degrees south of the equator. The population was not stable and, to meet educational needs in such a circumstance, if the people put up a structure that would keep out the weather, and found someone who could read and write and do a bit of figuring, the Government would pay his salary. There were aborigines all round, but it did not occur to me or anyone else that their young should attend school.

Here for four years I was happy. My kids gave no trouble and discipline was unnecessary; I taught them during the week and at week-ends they taught me. I could keep a horse or two, find plenty of game – turkeys, duck, pigeons – catch small fish all the year round and in the season make some incredible hauls of barramundi, the giant perch of India.

Up-country, far apart, were cattle stations and gold mines. Our police constable, with his half-dozen black troopers, had many errands to do and would often arrange for a trip to fit in with my holidays so that I could go with him. Thus I saw a good part of the Peninsula with no risk and little discomfort. I came to admire the self-reliance of the man – prospector, mailman, or what-not – who would start off with a saddle-horse and one or two pack-horses for a two-hundred-mile journey, on the whole of which he might not see a fence. I remember sitting by two men as, on the pub verandah, they discussed such a journey one of them was about to make. The other had already travelled the route and described it to his mate with as much care as if he were a scientific man describing

a laboratory experiment. Their language was full of strange idiom that to them was as valid as the technician's jargon would be to him. I liked it, for I was already, though unwittingly, interested in the relations of words to things.

I might have grown old on the Laura had not the powers in Brisbane promoted me to Townsville as assistant in a big school. I disliked the change. After my bush experience I could neither maintain discipline nor submit to the kind I met with and, having saved some money, quit teaching and came back to London to follow an old desire and study medicine. I knew I should be hard up, for I could not expect much parental support, but I had luck with an entrance scholarship at the London Hospital and in varying ways luck held till I qualified.

I appreciated the intellectual stature of my first teachers – Peter Chalmers Mitchell, Arthur Keith, and Leonard Hill, who all achieved knighthood, and Hugh Candy, through whose stimulus chemical theory so appealed to me that I was thrilled by the story of how Kekulé visualized the benzene ring. Whether that ring existed did not matter, but, by behaving as if it did, we had built up our knowledge of the aromatic carbon compounds.

Then I heard a lecture about Ehrlich's side-chain theory of immunity and I was puzzled. What led him to it? What use was it? There was surely something about it I had missed, but I hadn't the pluck to ask. It was years before I found comfort in von Haller, who had written in 1747, concerning some current theories: "We shall refer them to those hypotheses which the desire of explaining those things, of which we are unwillingly ignorant, has given rise to." At last I realized why Ehrlich produced his side-chain theory. It has already been forgotten, and so have others like it in the last half-century. But still they come.

This uneasy, almost obsessional, urge to satisfy myself made medicine hard going for me. My chief or my house physician would percuss out a stomach and mark it with blue pencil; but when I tried to do it for myself I couldn't. Percussing a heart, too, for deep and superficial dulness, left me unsatisfied. I am not consoled for my past discomfiture by knowing that no-one can percuss out a stomach, and that the distinction between the two areas of cardiac dulness, if it ever existed, is now lost. Of course no-one knew that this was the dark age of psychology in medicine, though an indication of uneasiness was shown by the common lament that, with the advance of scientific medicine, the old-style practitioner who 'knew his patient's constitution' was disappearing. Henry Head, the famous neurologist, was groping, and once said something about Pierre Janet. But 'Head says it's functional' would mark the end of general interest in a case and yet leave me feeling there was something more to it that I ought to know. The relative certainty of surgery as

compared with medicine attracted me, and it is now a favourite theme of mine that surgeons make better psychologists than do physicians. Herbert Page and Wilfred Trotter will serve for examples, and our orthopaedic surgeon, Tommy Openshaw, a Lancastrian, short and sturdy in body and speech, once left me profitably guessing. A woman stood before him with a peculiar sore on her arm whilst he questioned her about the possibility of anything rubbing it. Then he looked at her for a while. At last, "Sit down," said he; "Is your husband good to you?" When she burst into tears he patted her on the shoulder, told her "Your arm will get well now", and led the boys away without another word.

In 1902 I qualified and family affairs took me back to Queensland. Returning in 1904 I did house appointments at the London, serving as ophthalmic as well as general house surgeon, and took my F.R.C.S. in 1907. Through a London Hospital colleague in Shanghai I was offered a partnership there, and had the fortune to see Old China before the 1911 revolution, Shanghai was said to be 'A nervy place', and I can retrospectively diagnose psychogenic conditions I then failed to recognize. Life for the foreigner was luxurious and artificial, and in "An Examination of Tropical Neurasthenia" (*Proc. Roy. Soc. Med.* 1933, 911), I contrast the psychological effect of life there with life in North Queensland, where so-called neurasthenia was scarcely known.

By 1913 Shanghai had palled on me and on a wedding tour I travelled by Manila and Timor to Australia, on the way making a call at the Laura, where I found old pupils rearing another healthy generation in the tropics. Both my wife and I felt drawn to the old country, and in 1914 we set off with the intention of settling down in practice somewhere in the south of England. War broke out whilst we were at sea and as we came up the Channel we heard the distant thunder of the Battle of the Marne. At the London I was given a war-time job as surgical registrar, but in November I became a temporary lieutenant in the R.A.M.C. and was made surgical member of a board for officers in London.

The existence of war neuroses was then unsuspected, and I was taken aback when officers rolled up before us with obvious hysterical conditions, as well as the less definable troubles soon to be known as shell-shock. With scarcely an exception they brought diagnoses of some physical condition that for me did not exist. Bewildered, I talked about it with the late E. G. Fearnsides, a disciple of Henry Head, and suggested that our colleagues feared that we, a military board, might condemn the victims if we were told the truth. At the time I refused to believe him when he said they knew no better. "Frost-bite", later called "trench-foot", was then causing many casualties. E. G. F. and I studied some thirty cases in hospital and wrote a paper for the *British Medical Journal* (January 9, 1915) asserting that most of them were hysterical. This was

the first article in our journals about the war neuroses. Meanwhile I was impelled to send in a note to the higher ones telling them what was happening, and my appointment was soon cancelled. I can now recognize that I was quite unsuited for the position.

Next I went to a hospital near Portsmouth as surgical specialist, where my surgical work received credit but my diagnostic ventures were less successful, as a case or two will show. I found a man in my wards with a condition labelled *tubercular disease of the tarsus* but really an hysterical club-foot, for which I could do nothing, and after a month or two I persuaded my C.O. to have him invalided. He assured me that the War Office would not accept the diagnosis of hysteria and, under protest, I headed Army Form B.149 with the false diagnosis, though in the body of the paper I made clear what I thought of the case. The board procedure went through, but our A.D.M.S., Colonel Jennings, returned the papers marked "referred for further treatment". This stirred me up and, prepared for battle, I sought him out. He greeted my opening splutter with "Sit down there, young feller", and told me he recognized the situation and that the man must be invalided out with the correct diagnosis. I was full of gratitude when, only a few weeks later, his new M.O. wrote to me and asked why the man had been discharged, for he had put his foot flat on the ground again and re-enlisted. It would have hurt me if I, a Fellow of the College, had had to confess that I had put my name to the diagnosis of tubercular disease. In spite of this episode, when I sat on a board before which an identical case turned up, though with the label *talipes varus following osteo-arthritis*, the same C.O., as chairman, became angry when I refused to put my name again to a false diagnosis, and declared: "If you won't pass him out you can have him in your own ward."

Let me try to make clear the significance of these cases. Each man had inverted his foot by muscular action and walked on its outer edge as if he had one kind of club-foot. Anyone can do that. His hysteria enabled him to maintain the deformity, and some swelling followed because of immobility in a dependent position. Each had spent months in hospital where, with his fictitious disease, he fitted into the scheme of things; his being invalided with that disease would have fitted too, and no one would have worried, not even the hysteric himself, if the disability had remained for the rest of his days. Life would have been smoother for me if I had not interfered with a false diagnosis that made the road, wherever it led, so easy for all. I did not realize that my interference posed a problem no one cared to face, though my own helplessness when called upon to treat these two men should have warned me. There were no "trick-cyclists" to send them to in those days. The false diagnosis was the easy way out, and I insisted on blocking it.

Colonel Jennings had a shrewd idea of the situation, and at his request

I drew up some notes about hysteria, a copy of which he sent to each M.O. in his command. The M.O.s of neighbouring units now sent their problem cases to my hospital for an opinion, and I developed a technique for such as needed it. After making a diagnosis I would take the man into a side room where, with an opening "This is man to man", I told him, as best I could, my opinion as a surgeon, and invited his. A few abused me handsomely, some became expostulatory, an unexpected number thanked me and declared it was the first time anyone had taken that much interest in them, whilst occasionally one broke down and, with some emotion, told his hard-luck story. Among other things I learned that a man might have an hysterical symptom and yet be a decent and reasonable citizen.

It fell to me to inspect neighbouring auxiliary hospitals and convalescent homes, where on a quick run around I always found enough to confirm my belief that the conditions I was concerned with were widespread. Some typical states that at first required lengthy examination, and perhaps watching, before I could be sure of my diagnosis, had now become so familiar that I could recognize them almost at a glance, but this facility did not help me to convince a sceptical colleague. I was still playing a lone hand.

In 1916 I went to France and had my fill of surgical work from Paschendaele and the Somme. Shell-shock interested me, and I read about the after-effects of concussion, about separated synapses, dissociated cerebral centres, punctiform haemorrhages, gas-poisoning from high explosives, and other fantasies no nearer to reality than was the explanation of thunder as the voice of an angry god. I saw the shell-shockers who were sent down from the firing line to be shipped straight to Blighty, and I had no need for fantasy. I had not learnt how to spell psychology, but I could see that here was something the man himself was doing. Like Stahl two hundred and fifty years earlier, I would not put upon the material sciences more than they could carry.

In November, 1917, I came home on leave and, by the help of old friends at the London, obtained interviews with a few officers in high places. To one I gave some examples of typical and costly errors of diagnosis and was answered with "Yes, I dare say there are dozens like that, but we must cut our losses." When I told Sir John Collie, then powerful in the world of pensions, that one-third of his pensioners were wrongly diagnosed as suffering from organic disease, he said, quite thoughtfully, "Yes, that's rather an under-estimate. But if you try to press this you will make yourself very unpopular." What I then thought to be cynicism was a statement of fact by a very wise man. Colonel Aldren Turner, a well-known neurologist, altered my career. "This war," said he, "has revealed some lamentable defects in our medical education." He

went on to say the Army had more surgeons than were needed, but he could not get people to help with the cases we talked about. Would I give up surgery for the time being and help him? If so, he would send me to Maghull for a course of training.

I wondered what training I needed in a subject I had been studying for three years, and, armed with scepticism but impelled by curiosity, I went to Maghull. I soon knew that Captain Bernard Hart, Professor Pear, and Major Rows had something to teach me. I had read a brilliant article by Charles Mercier ridiculing Freud's theory of the unconscious, and not a word on the other side. That theory, honestly and impartially set forth, among others, now gave the key to many of my perplexities. Janet's theory of dissociation I have always been able to accept as a useful alternative, not contradictory, in one group of cases. Neither, however, dealt with what I still believe often happens – the cultivation of an hysteria that could be arrested by early recognition and simple handling. The spirit of Maghull was such that I was encouraged to give a lecture propounding my own ideas about this. I still value Bernard Hart's compliment that he had never known anyone else grasp theory as quickly as I did. But of course the theory was easy. It was the benzene ring again. It worked.

My only grievance against my teachers is that they gave no warning about hostility to the new views. My training was suddenly cut short and, with a fellow trainee, I was sent to Birmingham. A weary journey landed us at the University building, then the main hospital, and we thought our troubles were over for the day. A major in the office, however, unemotionally told us we would go on to Monyhull. "And how?" we asked. "You'll get a tram at the bottom of the road." Carrying our gear, and thirsty as well as tired, we went on to Monyhull, to be received rather nervously by a young captain who told us afterwards what a ferocious pair we looked. He explained that 250 neurasthenics had been hurriedly sent there from London because of air raids, and said how glad he was that we had come. Then he passed on the instruction that we were to give anaesthetics for the visiting surgeons, and we had to make it clear that we would give anaesthetics for no one. In his book, *Shell-shock: 1914–18*, C. S. Myers cites this instruction as typical of a general attitude.

Our wards contained a gibbering, fightable, mob. Annoyed at having to receive them, the authorities had decided that what these men needed was discipline and physical jerks. When we arrived no N.C.O. dared go near them, and the ward sisters told us they did not join the Army for this sort of work. After we had seen a few patients individually in our rooms, word was passed round that we meant well. The men settled down and began to respond to treatment, whilst the sisters came over to

the side of righteousness and stood by us bravely. The general situation was soon clear; our patients, when not insane, were liars and malingerers; we were credulous fools for taking them seriously and, being labelled psycho-analysts, were outcasts not entitled to benefit by the rules of the game. There were comic doings in this private war, and tragic. After some months a crisis brought Colonel C. S. Myers down from London. My colleague, a burly Scot, was beyond pacification and, refusing to go into hospital as a neurasthenic, was hurriedly invalided out. Colonel Myers took me to the Midland Hotel, gave me a good dinner, with good wine, and so far overcame my own truculence that I consented to stay on till he could send a relief.

In the autumn I was transferred to Ewell military hospital, where, with Lt-Col Abdy Collins as C.O., and cheery fellow-workers like S. E. Martin and J. F. E. Prideaux, I found peace and good fellowship.

Whilst at Ewell I gave some lectures at the London on the war material, and later on I was given a Lectureship on the Psychoneuroses, a pleasant task that I have only lately relinquished to Dr Henry Wilson. Demobilized in May, 1919, I took my M.D. at London University and published my degree thesis as *Psychoneuroses of War and Peace*. Narrow in its outlook when compared with more modern writing, it described methods and results of abreaction that will stand comparison with anything achieved today by the use of drugs.

I now settled down to practise as a psycho-therapist, though at the start my income came from work done for the Ministry of Pensions. About this time I underwent an instructional analysis, and learned what it felt like to experience the emotion of belief when my own unconscious material came to the surface – a necessary experience for one of my make-up.

In the early 'twenties the Industrial Health Research Board asked Miss May Smith and Eric Farmer, industrial psychologists, to investigate telegraphists' cramp. Approaching it as a fatigue phenomenon, they came to think it had a strong psycho-pathological element, and I was brought in by the Board to confirm or confute this idea. This meant the examination of many cramp subjects, who turned out to be suffering from severe psychoneuroses upon which the cramp was developed in response to the external situation. Although at the time we judged it best to reserve the observation, in some cases symbolism as described by Freud was obviously playing a part in a specific symptom. Even with this caution, our joint conclusions were strongly contested by some members of the Cramp Committee of the Board; but our opponents were scientific men in the true sense of the word, and after fair argument our report was accepted and published. (See "A Study of Telegraphists' Cramp", by Smith, Culpin and Farmer. *Ind. Health Res. Board Rep. No. 43*, 1927.)

We found an unexpected number of psychoneurotic subjects among the non-cramp controls, and the question arose whether a similar incidence would be found in the general population. This was important, and the Board encouraged May Smith and myself to find out the answer. Here began a working partnership that lasted for years and opened up an entirely new field. The staffs and subordinate workers in Government departments, private firms, training schools and colleges, laundries, the National Physical Laboratory, wherever we could get a foot in, we investigated by the personal interview, my colleague doing her share of interviewing whilst carrying out the McDougall-Schuster dotting test, which statistically corroborated the results of the interview. We discovered much that is now common knowledge, but our findings that about 30 per cent. of people at work suffer from nervous symptoms that interfere with happiness or efficiency, and that the symptoms of 6 per cent. to 8 per cent. are of a severity that sends people to the consulting room of the specialist, took a long while to soak in. Some of our results were published in "The Nervous Temperament in Industry", by Smith and Culpin; *Ind. Health Res. Board Rep. No. 61*, 1930, and have been amply confirmed by Dr Russell Fraser's recent work. A side-product emerged years later when, in connexion with A.R.P. work, the question was raised as to the number of claustrophobes in the population. Reference to a few hundred of our old case-notes gave the answer within an hour; about 6 per cent. of us have claustrophobia of sufficient severity to influence behaviour.

Out of these interviews there naturally grew a questionary that was speedy yet comprehensive. This I have taught and lectured about but, in spite of many requests, have refused to publish. It would have seemed as if all that was needed was to ask the questions and put down the answers, whereas each question was a stimulus that might or might not open up territory that needed exploring.

Sweeping statements about the psychological bad effects of noise led to demands for investigation. We spent a long time searching for such effects without finding anything much, except that many nervous people can't stand it, although in our search we temporarily deafened ourselves in the building that housed the experimental wind-chamber of the N.P.L. at Kew.

My advice came to be sought occasionally by official bodies, and it was to regularize the position that a part-time Professorship in Medical Industrial Psychology was set up and a home was found for me at the London School of Hygiene and Tropical Medicine. Miss (now Dr) May Smith shared this home with me. Our relationship was never defined and no definition ever seemed necessary; each of us always knew what was the other's share in any task we did together.

Industrial sickness next drew our attention, and here again our findings were contested, only to be confirmed later. This work involved the same principle as did my early struggles in the Army, for we found that most psycho-neurotic sickness was camouflaged as physical disease.

About 1930 I was invited to assist the Miners' Nystagmus Committee of the Medical Research Council. This nystagmus, an oscillation of the eyeballs occurring in coal-miners in safety-lamp pits, probably arises because of bad illumination. The Committee had already made two reports, but the situation was unsatisfactory. The disorder was scheduled according to a Compensation Act; that is, a man disabled by it was legally entitled to compensation. It was treated as an ocular disease within the sphere of the ophthalmologist, though a study of the literature showed that the oscillation played a dubious part in the disability. A large number of men with well-marked oscillation continued to work and made no complaint; in others claiming, and receiving, compensation, the oscillation could only be detected by an expert. Moreover, all the disabling symptoms, except a subjective movement of objects that occurred in some men, were typically psychoneurotic. This, already partially and dimly recognized, was explained by the theory that the nystagmus, working through channels unknown to the physiologist, produced the condition then called neurasthenia.

I examined 36 miners receiving compensation for nystagmus. Two of them showed notable oscillation and complained only of movement of objects. The others showed less oscillation, and in 22 I could detect none at all – not being an expert on that point. But the 34 were such a group as would be found at a clinic for psychoneurotic ex-servicemen, or among the sufferers from telegraphists' cramp. Unlike telegraphists' cramp, the oscillation is not a development arising from the psychoneurosis but serves as a peg upon which that disability can be hung. Otherwise it has no relation to it either as cause or effect. Even the movement of objects may fall into place as produced by a psychogenic spasm of accommodation, acting as a conversion hysteria.

For an account of this work see "The Occupational Neuroses (including Miners' Nystagmus)". *Proc. Roy. Soc. Med.* 1933, 655.

This view, challenging an assumption that had lasted for generations, could not be easily accepted. Ophthalmologists had never studied the disabling symptoms. One, from a mining area, when I explained to a committee that in a specific compensation case the man was disabled by severe phobias, replied that he ought to have his compensation stopped and be made to go down pit. I could only say "You terrify me!" The miners, with some excuse, regarded my denial of physical disease as an assertion that there was nothing the matter with them. Knowing that it is not good therapy to make a psychoneurotic believe he has a physical

disease, and then keep him idle and pay him for having it, I pleaded for the provision of surface employment for the man disabled by "miners' nystagmus"; but that was nobody's business. I have lost touch with the subject now, but during the last war it was plain, from newspaper reports, that Bevin boys who were psychologically unfit for underground work, and tried to say so, were being sent to prison.

Meanwhile, I and my working partner profited by our association with the staff of the School of Hygiene, where we had a class for the post-graduate Diploma in Industrial Psychology, also holding an informal clinic for industrial misfits. I lectured to the candidates for the diploma in Public Health, and, separately or together, we talked to various gatherings all over the country. I carried on my private work and turned out two more books on the medical side of my subject. I ought to have noted that in 1920 I published a book on spiritualism, for Janet's theory of dissociation had given me an understanding of how honest people could believe they possessed occult powers, including that of water-divining. Possibly it is my nineteenth-century materialist upbringing that still makes me unable to accept the evidence in favour of extra-sensory perception that satisfies some of my colleagues.

The nomenclature of the minor mental disorders, or *benign psychoses*, as, by analogy with malignant and benign tumours, I have tried to call them, with its neuropathies, neurasthenias, nervous debilities, psychoneuroses, neuroses and what not, had plagued me from the start of my interest in them; so I set out to explore the meaning and trace the origin of that queer word *neurosis*. I found many meanings and for its use got as far back as Cullen in the eighteenth century. This started us on the history of psychology in medicine, and here Dr May Smith and Professor Greenwood did some useful research, in which they re-cast the reputation of Stahl, maligned as 'the last of the animists'. I made this history the theme of my Presidential address to the British Psychological Society at Exeter in 1945. Without some knowledge of it one cannot understand current differences of opinion, based as they are upon unrecognized but basic differences of outlook, and, if I were confined to one lecture to students of psychological medicine, I should devote it to this part of the subject.

I learned the terminology of the technical parts of industrial psychology, such as time-and-motion study, job analysis, vocational guidance, the effect of rest pauses and hours of work, incentives, and so on, and as chairman of the Industrial Section of the British Psychological Society sometimes let people think I knew more than I did. These subjects were efficiently dealt with, when necessary, by my partner. The first item of my questionary, "How do you like your job?", however, gave me an insight into several matters. The answers were statistically related to

sickness absence, and there was revealed a corporate spirit, good or bad, in almost every organization. In one case the question found the clue to a specific example of industrial unrest.

There had been complaints of an excess of gastric illness among bus drivers, and statisticians were trying to find out if they were justified. As gastric trouble was coming to be recognized as possibly psychosomatic – that is, the physiological result of psychological factors – I got permission to examine bus drivers under treatment at the Trades Union Hospital, Manor House. Now at that time lightning strikes were frequent at different depots, always against the advice of the men's union and generally about some apparently trivial matter. My opening question drew forth dramatic descriptions of driving through London, of the need for watching traffic all the time, of watching lest a passenger should be shed by accident when the conductor was up aloft, of being harassed by police regulations, of the feeling of being under observation by an unrecognized inspector, of the supposed need for keeping to the schedule, of the shortness of the rest-spell at the end of the trip. "It's a job for a young man", was the refrain of the over-forties, and one man finished his grouse with "They pay you so well that if you chuck the job there's a dozen chaps ready to take it on" – a point of view I recommend to those who study incentives in industry. Here was an example of constant forced attention, already detected as a cause of stress in the spinning industry, when men engaged in the apparently easy task of watching for broken threads were a source of trouble until an industrial psychologist threw light upon the strain to which they were subjected. It seemed to me that these men, unable to summarize their trouble and make it understood, were expressing it in their irrational strikes. Soon afterwards an official inquiry into the men's grievances was set up and I was invited to present this idea of constant forced attention. Although the request came from the men's side, I regarded myself as a scientific witness helping on the inquiry; but the atmosphere was that of a vigorously contested case in a court of law. I was treated as a hostile witness by the employers' representative, who suggested that these sick men, having nothing else to do, had got together and, "by auto-suggestion", produced a confabulation which, in my innocence, I had mistaken for truth. This was quite like old times and it gave me an unfavourable impression of human relations in that branch of the transport industry. It did more. I had read Elton Mayo's *Human Problems of an Industrial Civilization*, in the light of which this case made me see that the task of industrial psychology, aided by the study of human relations, is to make the wheels of industry go round.

Looking back on this my story, I see that I was unwittingly sharing in a change of outlook, not only in my own profession, that will become

of historical importance. I have described the part in which my little world was concerned, but there were doings more important. The pioneers of work on the war psychoneuroses included W. H. R. Rivers, Elliott Smith, William McDougall, and C. S. Myers, all Fellows of the Royal Society and accustomed to the amenities of an academic world, who wrote and talked about the new ideas, but retired from the field when the World War was over. Myers, in his book already mentioned, tells frankly why he abandoned the subject, and McDougall's personal friends knew why he left Oxford for North Carolina. Freud, like Copernicus and Darwin, was a disturber of the peace, with his demonstration of a new world where the mechanical sciences wouldn't work. By bringing sex into the daylight he broke the most powerful taboo of our society, and the opponents of any psychological approach took full advantage of this to obscure the issue. The real conflict, as Myers and McDougall must have felt it, and as it now appears to me, was between a dynamic psychology and the older mechanistic conception, a conception that, potent because unspecified, was the basis of my own education.

I turn to present-day affairs to stress the changes that have taken place. Psychiatry has advanced beyond the study of each psychoneurotic patient as a self-contained problem, for he is modified or produced by family and social conditions, and his happy adjustment to society is the final aim. In this last war the psychiatrist was given tasks and responsibilities that would have been outside the conception of my teachers at Maghull. In co-operation with the industrial psychologist, he took part in the selection of officers, the grading and distribution of army misfits, and the study of problems of morale in a citizen army. Industrial psychology, though still concerned with the adjustment of the worker to his task, has joined with social psychology in studying the problems of industrial relations that are every day becoming of greater importance, when viewed in the light of world developments. All this should make us wonder whether man will ever try to solve world problems by studying himself with the same objectivity that he has used in the study of the atom.